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PRACTICAL INFORMATION

ON THE

MALIGNANT SCARLET FEVER

AND

SORE-THROAT.

IN WHICH

A NEW MODE OF TREATMENT

IS FREELY COMMUNICATED.

By E. PEART, M. D.

LONDON:

PRINTED FOR W. MILLER, OLD BOND STREET.

1802.

PREFACE.

IT is common, in treating on any diſorder, to give its hiſtory: to detail minutely all the ſymptoms which occur, whether eſſential and characteriſtic, or accidental, and common to many diſorders: to treat of the diagnoſtics and prognoſtics: to inveſtigate the remote and proximate cauſes; and, then, to point out the indications of cure, and enter upon the treatment of the diſeaſe.

This regular mode of proceeding it is not my intention to follow.

The hiſtory will be given in a few words: the prominent ſymptoms which characterize the diſorder in queſtion, will be briefly ſtated; and the ſimple treatment which has proved ſo eminently

ſerviceable, will then be clearly deſcribed, without entering upon theory or conjecture.

Medical theories have been numerous: but I am afraid they have been more prejudicial than uſeful; as they have, too commonly, led their converts to modes of practice, which have had no foundations, but the theories themſelves: and as all thoſe theories, being eſſentially different from each other, cannot be right; of courſe, all the modes of practice purely theoretical, muſt be injurious, except thoſe which are founded on the *truth*: and the true theory of diſorders is far from being determined upon; as is evident from the jarring opinions of the various profeſſors of medicine, even at this preſent time.

It appears extremely probable to me, that one great ſource of error, and one great reaſon why real valuable knowledge is ſo little advanced, is, the want of ſtrict attention and perſeverance in the purſuit of truth. It has been too common a practice, and ſtill is ſo, to admit of effects as cauſes; and, when difficulties preſs, to ſubſti-

tute words for ideas, and to adopt terms which are unintelligible, as legitimate and ſatisfactory.

As an example of what I have now advanced, if you caſt an eye over the celebrated theory of Cullen, you will find every where *ſpaſm*, and the *vis medicatrix naturæ*.

He makes ſpaſm to be the proximate cauſe of fevers, and inflammations in general. Admit, if you pleaſe, that it is ſo; ſtill ſpaſm is not the proximate cauſe of thoſe *diſeaſes*, in which fever and inflammation occur; becauſe *ſpaſm* is a morbid *ſymptom* which ariſes from an adequate cauſe: nay, even the *cauſe* of *ſpaſm itſelf*, does not conſtitute the diſeaſe, as that cauſe does not invariably produce ſpaſm, but when it *acts on diſordered parts*.

For inſtance, in ſome conſtitutions vegetable acids in the ſtomach produce reſtleſs nights, dry ſkin, feveriſh heat, and violent cramps of the feet or legs. Spaſm is evidently the cauſe of the pain

pain of the muſcles of the extremities, and probably of the dry ſkin, feveriſh heat, and reſtleſſneſs; but this ſpaſm itſelf is but an *effect*, cauſed by the *acid* in the ſtomach, which excites the *nerves* of that organ to *morbid action*, which being communicated to the *ſenſorium*, is productive of morbid action there; and from thence it is propagated to the ſurface, and alſo to certain muſcles of the feet or legs.

As the acid in the ſtomach, then, is the cauſe of ſpaſm, by removing that cauſe, the effect will ceaſe; and in ſuch caſes, a ſufficient doſe of magneſia taken at bedtime, will prevent or cure the ſpaſm, by neutralizing the acid, as I have experienced a thouſand times.

But although the *acid* be the cauſe of the ſpaſm, ſtill it does not *conſtitute the diſeaſe*; that, in fact, is that *peculiar ſtate* of the nerves of the ſtomach, and of the ſenſorium in conſequence, which renders them *ſubject* to *morbid actions* when acted upon by *acids*, *which*, otherwiſe, and in other ſtates of the ſtomach, are *innocent*.

It

It may be ſaid the *ſpaſm* is a *morbid action*, but the *acid* is but an *exciting cauſe* of that morbid action. So alſo it may be truly ſaid, that *fever* is a *morbid action*, and *ſpaſm* but the exciting cauſe; for ſpaſm is not fever; but it, in ſome caſes, ſo deranges the actions of the ſyſtem, as to be productive of that *degree* of *diſorder*, which conſtitutes *fever*. In ſhort, the nerves are liable to become diſordered in *various ways*, and are ſubject to the action of a *great variety* of ſtimulants; conſequently, irregular actions, ſpaſms, and vaſcular diſorders of *various deſcriptions*, muſt enſue.

But as ſpaſms are excitable by *various kinds* of ſtimuli, acting upon nerves in *different ſtates* of diſorder, it is like ſaying nothing to refer all fevers and inflammations to ſpaſm; becauſe, to enable a practitioner to act effectually and rationally in removing ſpaſm, he muſt, firſt of all, know what is the *nature* of the *cauſe*, which excites the ſpaſm he wants to remove; and what is the *real ſtate* of the *nerves*, which renders them thus liable to ſpaſmodic action, in conſequence of the application of ſuch ſtimuli. This appears to me to

be

A proof of this occurs in the celebrated, and by many, greatly admired theory of John Brown.

According to his theory, *defect of ſtimulus* conſtitutes, in ſome caſes, *direct debility*; and in others it induces *increaſed excitability*. In the former caſes the quantum of ſtimulus muſt be increaſed, to remove the direct debility; but in the latter, the quantum of ſtimulus muſt be leſſened on account of the increaſed excitability. But it certainly is inconſiſtent to aſſert that defect of ſtimulus induces direct debility, without increaſe of excitability, in one caſe; and increaſe of excitability, without direct debility, in the other; becauſe, to be conſiſtent with itſelf, the want of the uſual ſtimulus ought *at all times* to produce the *ſame ſtate*, curable by the *ſame means*, though circumſtances may vary the degrees.

Dr. Darwin, in his moſt elaborate Zoonomia, gives nearly the ſame explanation of diſeaſes as John Brown; only, inſtead of excitability, he uſes the term—ſenſorial power. Dr. Darwin

ſays, that when the ſenſorial power is in its natural quantity, and there is a *want* of *ſtimulus*, *direct debility* is induced: and he likewiſe ſays, that a *defect* of *ſtimulus* induces *accumulated ſenſorial power*; which, by raiſing the ſtimulus even to the common ſtandard, brings on inflammation, &c. When the diſorder to be explained is really direct debility, and ſtimulants are wanted, then the *want* of *ſtimulus*, while the ſenſorial power is in due proportion, is the cauſe of *direct debility*. But, when violent action, or fever, or inflammation, is to be accounted for, then the *defect* of *ſtimulus*, the ſenſorial power being in healthy proportion, cauſes *accumulation of ſenſorial power*; which even by the *uſual ſtimulants*, is excited to *inflammatory action*. It certainly is an inconſiſtency at leaſt, thus to attribute *contrary effects*, requiring *contrary modes* of *treatment*, to the *ſame diminution of ſtimulus* as a cauſe, acting upon *ſimilar ſtates* of *excitability*, or *ſenſorial power*; and I therefore reject the ſyſtems founded upon ſuch principles, as erroneous. Neither can I ſee, as Dr. Darwin ſeems to inſinuate, that his having been led to form ſimilar concluſions with Dr. Brown, though from

different premiſes, is any proof of the validity of ſuch concluſions; till it be proved that men are more prone to hit upon truiſms than to draw concluſions, which do not, in the end, prove to be ſo valuably correct as at firſt they are thought to be.

It is not my intention to combat the popular ſyſtems of medicine, nor to ſay more than that they are imperfect and unſatisfactory, or founded upon erroneous principles; conſequently to abide by them is to abandon truth; and for that reaſon I avoid entering upon the theory of the diſeaſe I at preſent have choſen to write upon; becauſe, to adopt a language which conveys no information, or explanations founded on falſe principles, is to propagate error; and to ſport new opinions, while men are perfectly ſatisfied with an old one, is a ſure way to be branded with the epithets of innovator, and diſturber of public quiet.

For the ſame reaſons I avoid ſaying any thing about the indications of cure, or the *modus operandi*

operandi of the medicine I recommend; for, ſo long as the cauſes of diſeaſes are unknown, little ſatisfaction can be expected from the explanations which are given, of the manner in which medicines act in removing them.

That I may not, however, be thought to condemn without reaſon, I ſhall take the liberty of making a few general remarks on the popular ideas on the ſubject.

The *operation of medicines* in curing diſeaſes, is, in the preſent theories, greatly ſimplified; and, in every ſenſe of the word, the preſent explanations are *truly ſimple*.

The oxyds of antimony are powerfully emetic; —the oxyds of mercury excite ſalivation;—the oxyds of iron are tonic;—in fact, every metallic oxyd has its powers of action on the living ſyſtem, and that by no means inconſiderable.

The *French* chemiſts *aſſert* that all theſe metallic oxyds produce their effects by oxydating the

the animal fibres, or the nerves of the ſyſtem, or the fluids, or all of them: therefore, the *Engliſh* chemiſts *aſſert* that they act by imparting oxygen; and thoſe who have *no buſineſs* to *aſſert* at all *believe*, therefore, that all theſe powerful effects are produced by the oxygen of theſe metallic compounds being imparted to the vital fibres, &c.—Nothing can be more ſimple; and to thoſe who are ſatisfied with ſimple things, nothing can be more ſatisfactory.

The oxygen, imparted to the ſyſtem by antimony, cauſes *vomiting*, but no ſalivation. That imparted by mercury cauſes *ſalivation*, without being neceſſarily emetic; and the oxygen communicated by iron is neither emetic nor ſalivant, but ſimply *tonic*; unleſs the doſes be prepoſterouſly large—then, even wine itſelf may act as an emetic.

Now we are taught by the French, that every metal is a ſimple, uncompounded body; and it is evident that neither antimony, mercury, nor iron, are poſſeſſed of active influence upon the body

body in their metallic ſtates; at leaſt, their powers of action are incomparably leſs in their ſimple ſtates, than when combined with oxygen; for a man may eat a handful of iron filings, and waſh them down with a gill of mercury, without inconvenience; whereas a few grains, for inſtance, of mercury, when oxydated, are quite ſufficient at one doſe; conſequently, then, they argue, oxyds muſt act by means of the *oxygen*; and it muſt be the *oxygen only*, which produces the powerful effects in queſtion. The only difficulty which occurs in this matter is, to explain, intelligibly, why the *ſame* pure oxygenous principle ſhould, by *mere ſeparation* from theſe ſimple metals, ſometimes prove violently *emetic*, at others powerfully *ſalivant*, and on a third occaſion ſimply *tonic?*

Again, quickſilver is mild, inactive, and may be drank, like water, without ſenſible effect.

The muriatic acid may be taken to the quantity of a *drachm* or *two*, with water, without any inconvenience.

If

If to this muriatic acid you *add* a conſiderable proportion of the *principle of acidity*, it will become *leſs acid*, and may then be taken in ſtill *larger* quantities diluted with water, or almoſt without dilution!

If this ſimple quickſilver be put to this mild, or ſur-oxygenated muriatic acid, an oxyd or oxymuriate of quickſilver will be formed; of which, thoſe who take *one grain*, at *once*, will moſt ſincerely wiſh they had been content with *leſs*.

Now, if this grain of oxymuriate of quickſilver produces ſuch violent effects by ſimply imparting the third part of a grain of oxymuriatic acid, which it had combined with, to the vital fibres of the ſtomach; how happens it that five hundred times as much of the *ſame* kind of muriatic acid as that which was combined with the mercury, has no ſuch violent effects, nor ſcarcely any conſiderable effects whatever?

To avoid as decently as poſſible, difficulties like this when they impertinently intrude, as has been

been the caſe on other occaſions beſide this, it has been ſhrewdly inſinuated that the oxygen produces theſe prepoſterous and out-of-the-way effects only, when it is brought into action in a *naſcent ſtate*.—Happy, thrice happy the genius which ſuggeſted ſo fortunate a ſalvo, in ſo embarraſſing a dilemma!

Oxygen then, in its *naſcent ſtate*, is quite a different thing from oxygen *a day old!*

But, if oxygen be oxygen, and nothing elſe but oxygen, it ſtill remains to be explained, why, and in what reſpect, pure oxygen, *juſt ſeparated* from a body, *differs* from *itſelf*, being ſtill *pure oxygen*, at the *minute's end?*

Here, for the preſent, I ſhall drop the unpleaſant employment of finding out faults and conjuring up difficulties. I muſt aſſert, however, in the ſtrongeſt terms, that there is nothing invidious in any of the tranſient remarks I have made; on the contrary, I have the higheſt veneration for every name I have mentioned; and

eſteem their invaluable labours as juſtly entitling them to the higheſt honour, and to everlaſting fame! To equal celebrity and reſpect I think ſome few of the French chemiſts are juſtly entitled; at the head of which liſt is the name of *Lavoiſier*; than which none is greater at this day, nor has a fairer claim to paſs with honour through ages yet to come! But no name, howſoever great, can ſanction *error:* and it certainly is meritorious in any man to expoſe *it*, whenever he diſcovers *it*; that its baneful influence may be deſtroyed, that truth be no longer obſcured by its ſhade: and my chief motive for bringing forward theſe difficulties is, that by fair reaſoning they may be explained away; or, that principles, which *cannot* be *ſupported* by *reaſon*, may *not* be thought worth *defending* by *argument*; but, that ſounder principles be ſought for to ſupply their place.

THE

PRACTICAL INFORMATION,

&c. &c.

The Hiſtory and Deſcription of the Diſeaſe.

EARLY in 1801, ſeveral caſes of ſore-throat occurred, which were neither dangerous nor peculiarly obſtinate. In July many children were ſeized with ſickneſs, ſucceeded by fever, which in a few days was attended with an univerſal eruption, which by their mothers and nurſes was ſuppoſed to be the meaſles; ſome degree of ſoreneſs of throat at the ſame time was generally complained of. All theſe ſymptoms in a few days, moſt commonly, diſappeared, with-

out medical aſſiſtance being thought neceſſary.

It was ſoon, however, found, that the diſorder was not the meaſles, as the fever and ſoreneſs of throat, in ſome caſes, were particularly diſtreſſing, while the eruption was ſcarcely viſible; and ſeveral children, who had lately had the meaſles, were covered with a florid efflorescence, which after a few days diſappeared, when the cuticle gradually peeled off in ſcales like bran; and it now was found to be infectious, as it commonly ſeized upon all the children in the family, and ſpread from houſe to houſe; few entirely eſcaping, where there were children, in the neighbourhood. Some few amongſt the lower claſſes died, probably owing to miſmanagement: as hitherto there had been no ſigns of much malignity. I ſay probably from miſmanagement, as it is ſo common a practice with the poor in caſes of ſickneſs to beg wine and ardent ſpirits of their affluent neighbours, which they adminiſter to the ſufferers in all caſes indiſcriminately. I cannot mention this circumſtance without

without at the ſame time expreſſing a wiſh, that thoſe whoſe benevolence renders them ſubject to ſuch applications, would never give either wine or ſtrong liquors of any kind, till they have, what they at leaſt think to be, ſufficient reaſons for believing that ſuch things will not be injurious; but, if a medical man is employed, never without his approbation.

In Auguſt the diſorder became really dangerous and truly alarming; the fever, efflorescence, and ſore-throat ſpread rapidly, and was no longer confined to children: as now young adults of both ſexes were ſubject to its ravages; nay, in ſome caſes, but they were not numerous, thoſe who had arrived to the ages of forty, and even fifty years, were attacked, and that, too, with extreme violence.

The throat, now, frequently became highly inflamed, ſwelled almoſt to ſuffocation, and frequently ulcerated and highly fœtid; the parotids on each ſide alſo being greatly enlarged and very painful.

But

But the moſt diſtreſſing and dangerous ſymptom was an affection of the brain, which, particularly in adults, was productive of ſcenes of horror indeſcribably diſtreſſing! In theſe caſes the fever was high; the ſkin fluſhed, but not always efflorescent; the pulſe rapid; the throat commonly highly inflamed, often ulcerated and ſwelled till breathing was an herculean labour: but, the raging madneſs of delirium! oh, it was inconceivably horrid!!! Frequently it required the ſtrength of three or four men to keep the patient in bed: if for a moment left unguarded, he threw himſelf upon the floor, and rolled around in agony; and, if a female, was regardleſs of modeſty and inſenſible to ſhame. Sometimes the moſt miſchievous rage poſſeſſed them, and their neareſt and deareſt friends were not exempted from abuſe; while the loud ravings of frantic wildneſs, or the wailings of diſtreſs, aſſailed the ears, and ſtruck terror to the hearts of thoſe who paſſed before the houſes which contained the miſerable, the wretched ſufferers!

As may be ſuppoſed, unleſs the violence of the diſorder was ſpeedily ſubdued, a horrid death was ſhortly the inevitable conſequence; when a high degree of putrefaction rapidly ſucceeded, and, not unfrequently, conſiderable quantities of blood eſcaped from the diſtended veſſels, and poured out by various paſſages.

The pulſe was always rapid; the breathing quick, laborious, and interrupted by frequent ſighings; the ſenſation of diſtreſſing oppreſſion about the præcordia was anxiouſly complained of; and the dread of ſuffocation from the great diſtention of the veſſels about the throat, increaſed the miſery to the higheſt degree of agony.

Towards the cloſe of this ſcene of wretched ſuffering, children commonly became convulſed, and adults not unfrequently.

A variety of other ſymptoms occaſionally attended to add to the diſtreſs; but, as they were neither

neither conſtant, common, nor eſſential to the diſorder, I ſhall not ſtop to conſider them.

About this time ſeveral, particularly children, ſpeedily fell victims to its malignancy; ſome being taken off in eight and forty, ſix and thirty, or even, in ſome few caſes, within four and twenty hours from the time when the attack firſt became ſenſibly evident! In one village, as I was afterwards informed, between forty and fifty died of the diſorder within the ſpace of one month: a fatal proof of its prevalence and malignancy!!!

The duration of the diſorder, whether it terminated in health or death, was very various. Some were taken off rapidly; ſome ſtruggled to the ſixth or eighth day; and ſome even longer, before their powers were finally extinguiſhed. In ſlighter caſes patients commonly recovered in ſix or eight days; but in more ſerious attacks, the time of the diſappearance of the infection was extremely uncertain; and, when the brain had been conſiderably affected, ſtupor, fatuity,

and

and in ſome caſes a certain degree of general paralyſis were the unhappy conſequences. Sometimes fœtid ulcers appeared, and in ſeveral the extremities were much ſwelled, and remained ſo after the fever was ſubdued.

In Auguſt and September the contagion was moſt virulent and general, and conſequently moſt fatal; after that its influence gradually diminiſhed. But, even in the month of March 1802, ſeveral caſes occurred; and at this moment, June 1802, it is not totally extinguiſhed, although it now frequently takes quite a different form.

Having thus given a general hiſtory of the riſe and progreſs of this alarming, diſtreſſing, and fatal contagion, and of its uſual modes of attack, characteriſtic ſymptoms, and various terminations, as it appeared in the neighbourhood where I reſide, I ſhall avoid being tedious by entering into a detail of minuter and leſs important appearances, and ſhall immediately proceed to the mode of treatment.

Method of treating the Diſorder.

The firſt appearance of the diſeaſe was not marked with any ſymptoms of violence or danger; few died. And as many readily recovered when the conſtitution was unaſſiſted by medicines, and many under different modes of treatment, it is not clear that the medicines which were applied were of any real utility.

In Auguſt, when the infection became peculiarly virulent, and rapidly ſpread its baneful influence around; when death and horror joined company with the diſeaſe, and wild alarm was painted upon every countenance, it then became truly a ſerious conſideration, and the moſt ſtrict and unremitted attention became abſolutely neceſſary, that the moſt effectual mode of treatment might as ſoon as poſſible be aſcertained.

I found *by repeated inquiries* reſpecting the progreſs of the diſorder, and the fate of thoſe who were ſeized with it in the adjacent

cent villages, that bleeding, bliſtering, emetics, purgatives, ſalines, ſudorifics, acids, &c. with gargariſms of various kinds, had been reſorted to. Some recovered; ſome died; but none were ſenſible of having been really benefited by the means which had been employed; and it was ſoon ſtrikingly evident to the inhabitants around, that the proportion of recoveries by thoſe modes of treatment, were *not greater* than when the diſeaſe was entirely left to the conſtitution; and that the violence of the ſymptoms and duration of the diſeaſe were not *ſenſibly affected* by any thing which had been done to relieve them; therefore great numbers choſe to truſt to nature only.

I now was applied to by ſeveral labouring under the diſeaſe in an alarming degree, and in a few days had twelve patients, chiefly adults, beſides many more ſlightly affected. Nine of them had fever and efflorescence, with violent inflammation and ſwelling of the throat; the other three, in addition to the above complaints,

were attended with great affection of the brain, and conſtant and high delirium.

I, too, had recourſe to the eſtabliſhed modes of treatment, the effects of which I attended to with unremitted application; but I could not perceive that any medicine which I applied poſſeſſed the leaſt power over the diſorder, either in mitigating its violence, or haſtening its diſappearance.

The three patients who were ſo highly delirious became worſe! I changed my medicines; I tried every powerful medicine that was likely to ſucceed; I ranſacked my library for precedents, and conſulted all the authors of celebrity who had treated on ſuch diſorders; but ſtill not any one article, or any combination of medicines, had any evident or decided effect in either moderating or removing the diſeaſe!

Now, ſome of the patients of the nine who had not any conſiderable delirium, ſlowly recovered. One of the three in whom the brain had been

been ſo greatly affected, in defiance of all the pains I had taken to relieve him, died! and a ſecond, at the ſame time, ſhewed ſigns of recovery. As the third had been attacked later than the other two, and as the modes of treatment in the two former were not exactly the ſame, I immediately put him under the ſame courſe of medicine as had been adminiſtered to the recovering patient; but, alas! with no good effect, as he alſo fell a victim to the diſeaſe on the ſecond day after!

The others ſlowly and difficultly recovered; but after a moſt impartial review of every circumſtance, and a fair compariſon of theſe caſes with the general progreſs of the diſeaſe when left to nature, or under different modes of treatment, I was under the neceſſity of drawing this mortifying concluſion, that all my attention, and all the medicines, however various, or in whatever manner combined, which I had hitherto given, had *not* been *productive* of *any good effect whatever!*

At

At this very moment, when all my efforts were baffled! my confidence deſtroyed! and hope itſelf almoſt extinguiſhed by the diſtreſſing load of anxiety which oppreſſed my mind, the diſorder became ſtill more formidable, more frequent, and more commonly alarming! Every day new names were added to the liſt of ſufferers; and it was not without the moſt mortifying and painful ſenſations that I ſaw them fly with confidence to me for relief, at that very time when experience had juſt taught me by the cleareſt evidence, that *ſmall indeed* was the *aſſiſtance* in *my power to give them*; unleſs by a *bold deviation* from the eſtabliſhed modes of practice, an *effectual remedy* could be found.

For ſome time paſt I have not only made ſo free as to think for myſelf, but have alſo ventured to think very differently from others upon many ſubjects. The nature of contagious diſeaſes was one of thoſe ſubjects; and as my ideas of the cauſe were widely different from the common opinion, ſo alſo is the practice which thoſe novel ideas ſuggeſted.

The

The remedy which my ideas inſinuated as moſt adapted to the removal or deſtruction of contagion like this, however, was merely *theoretical*; and as ſuch, I could not think myſelf authorized to try experiments with it, while eſtabliſhed modes of practice were ſucceſsful. But by the deaths of theſe two patients, and the decided inefficacy in every other caſe of ſevere affection, I was now perfectly convinced that thoſe *eſtabliſhed modes* of treating the preſent diſorder were neither *ſucceſsful*, nor in the leaſt to be *depended* upon; and therefore not only thought myſelf authorized to change, but alſo fortunate in having any promiſing reſource, in ſo diſtreſſing a conjuncture, to fly to.

In April 1800, I had had ſeveral patients in a *typhus fever*, which bore ſome ſlight reſemblance to this, and which evidently ſeemed to be communicable. It was not, however, either peculiarly dangerous, much extended, or long before it diſappeared. The uſual modes of treatment were, at that time, very unſatisfactory; and towards the decline of the fever, I determined to try the very remedy which I ſince have had recourſe

recourſe to. I gave it in three or four caſes; but the patients recovered ſo immediately after, that I could not think myſelf juſtifiable in concluding that the medicine had been any way concerned in their recovery; and as no more caſes of that kind appeared either then or ſince, of courſe the real powers of the remedy were ſtill unaſcertained.

Immediately, therefore, after the death of the ſecond patient already mentioned, I determined that the next caſe which proceeded, unchecked by the uſual remedies, to become really dangerous, or rather to become alarming, ſhould be ſubjected to the *new mode of treatment entirely*, which I was reſolved to adminiſter boldly; but, as the medicine in which my only hopes were founded was ſo diametrically oppoſite in its qualities to thoſe which are univerſally eſteemed proper in raging fevers, I, at the ſame time, determined to give it with prudence and caution.

It was evening: my reſolution had not been fixed one hour, when two meſſengers arrived

from two of my patients. The firſt was a gentleman upwards of fifty; the other a married woman, the mother of a family, aged about thirty. Each of them had been ſeized with a violent attack of the diſorder about five days before: the firſt had been my patient three days, the latter two.

The ſymptoms were very ſimilar, each having ſlight efflorescence; the throat much ſwelled and inflamed; and in both the fever had been high from the firſt, with great anxiety of mind and reſtleſſneſs.

As their diſorders had been ſimilar in their progreſs, ſo were their ſituations now; as the information I received was, that in each the fever was greatly increaſed, and both were reſtleſs and delirious. None of the uſual medicines which had been given in either caſe had been productive of any ſenſible effect upon the diſeaſe, which proceeded equally, and with unremitting or rather increaſing violence in both!

My reſolution was already taken; and the moment was ſoon to arrive which would determine the fate of my reaſonings, my remedy, and my patients: nay, not only of theſe, but probably of many beſide them; as numbers kept daily applying, either for the firſt time, or for more efficacious remedies than thoſe they had already tried in vain!

Without heſitation, therefore, I diſſolved *two drachms of volatile alkali, or carbonate of ammonia, in five ounces of water:* half of which ſolution was diſtributed to each patient, with orders to take *half a table-ſpoonful*, or *two tea-ſpoonfuls*, every *two*, *three*, or *four hours*, according to the *urgency* of the *ſymptoms*. If the difficulty of ſwallowing abated, and the patient wiſhed it, a little cold water might be added to each doſe. Cold water, or toaſt and water, to be drank at pleaſure. I particularly requeſted that I might be informed of the ſtate of both theſe patients on the following morning; and it was not without conſiderable agitation, produced by contending hopes and fears, that I ſaw the meſſengers arrive.

Conceive

Conceive then what was my ſurpriſe! how great my pleaſure! how extreme my ſatisfaction! when I was informed that each of my patients had found aſtoniſhing relief, even from the very firſt doſe of the medicine! that both had had ſeveral hours of refreſhing ſleep, the firſt they had enjoyed ſince the commencement of the diſorder; and, that both were cool and perfectly collected, having had nothing like delirium after the firſt doſe had been taken half an hour.

Now had I reaſon to hope that I was poſſeſſed of a remedy, which ſeemed to be endowed with a *ſpecific power* over the diſeaſe; as in theſe two caſes it had manifeſted an immediate action, by extinguiſhing the fever and ſoothing the mental agitation into perfect compoſure. In ſhort, it gave me pleaſure to find that theſe two patients were ſnatched as it were from a danger, which experience had taught me the uſual modes of treatment were by no means of any efficacy in removing; and the moſt exhilarating hopes that my other patients would ſoon experience the ſalutary effects of the remedy, and confirm its

power by becoming happy evidences of its ſucceſs!

Nor were my hopes diſappointed!—From this time the volatile alkali was my conſtant remedy in every ſtate, every form, and every ſtage of the diſeaſe.

Some were glowing with univerſal efflorescence; in ſome the extremities were ſwelled; in others fœtid ulcers appeared, particularly about the parts of generation; in moſt the throat was ſwelled and inflamed, often ulcerated, and reſpiration almoſt prevented: but, in the moſt alarming caſes, a ſcorching fever, and raging delirium, rendered the patient's ſituation horribly alarming; yet, in all theſe variations of the diſeaſe, the volatile alkali was my ſpecific remedy, which I adminiſtered to between two and three hundred patients ſucceſſively and ſucceſsfully.

In fact, although a great number of thoſe patients were afflicted with the very worſt ſymptoms of the diſorder; and although many of them

them did not apply till the diſeaſe had gained its utmoſt virulence, yet, under both thoſe diſadvantages, out of near three hundred who took the volatile alkali, but two died. Both were very young children:—in both the parts about the throat and noſe were extremely ſwelled and ulcerated:—in neither of them was the ſolution given in ſuch quantity as was *likely* to ſucceed:—in both the virulency of the infection was extreme, and far advanced before relief was applied for; one of them having been ſix days before application, and the throat was become ſo fœtid, and the child ſo generally and conſtantly convulſed, that I declared its recovery impoſſible; and only gave the medicine to ſatisfy the parents, without any expectation of its being of benefit to the child.

In recent caſes, the firſt or ſecond doſe very frequently entirely deſtroyed every appearance of diſorder: in moſt caſes its beneficial influence was more or leſs ſenſibly perceived from the very firſt; and the ſatisfaction which my patients in general expreſſed, when they came to inform me

me of its operation, was not leſs than my confidence in its powers, and my heartfelt pleaſure at its ſucceſs.

I mean it not to be underſtood, neither indeed can it be ſuppoſed by any intelligent perſon, that the volatile alkali was the *only medicine* which was adminiſtered in all theſe caſes. In the greateſt number of them, however, it was the *ſole remedy* which was given, and all that was found neceſſary to the ſpeedy and perfect removal of the diſeaſe, and the re-eſtabliſhment of health; and in every other of theſe caſes, its powerful and beneficial effects in removing the fever, &c. were not leſs evident. But peculiar circumſtances and conſtitutions were liable to various diſtreſſing ſymptoms which were *excited* by the diſeaſe, but by no means *eſſential* to it, or its conſtant attendants; and theſe accidental ſymptoms, in their turn, kept up the diſorder, and required particular attention, as well as the original diſeaſe; that, as common diſturbers, they might be expelled together.

As

As my only intention at preſent is to point out to others the remedy which has proved ſo ſingularly and extenſively ſucceſsful in the ſcarlatina maligna vel anginoſa in my private practice; I ſhall not enter into a ſpecification either of the varieties of thoſe accidental ſymptoms, or the kinds of medicines which were neceſſarily combined with the ſolution to remove them. Whatever be the diſorder which a patient is attacked with; howſoever well known or common, it is, at all times, to his intereſt, his ſafety, and ſatisfaction, to apply to the *moſt intelligent and expetienced practitioner* within his reach; and to ſuch this ſpecification would be unneceſſary. On this head I will only further obſerve, that as I never found any advantage was derived from either bleeding, bliſtering, emetics, cathartics, diuretics, ſudorifics, opium, bark, acids, or any kind of medicine whatever, beſides the volatile alkali, when the diſeaſe was *ſimple*; of courſe, I never had recourſe to any other remedy, until I was convinced that the *contagion* was *deſtroyed. Then*, if troubleſome ſymptoms attended, I immediately *conjoined* ſuch medicines as I thought neceſſary

neceſſary to remove them; when the ſymptoms, and the diſorder, which they kept up by increaſing the general diſtreſs, diſappeared together; or, if ſtupor, fatuity, anaſarca, general debility, or even delirium, without any remaining fever, as frequently happened, were conſequences; they readily yielded to appropriate remedies, without any difficulty that I ever experienced.

Having thus faithfully laid open my experience; the new mode of treatment, or rather the new remedy I have adopted, and the great and conſtant ſucceſs which has attended it in nearly three hundred caſes in every ſtage, every form, and every degree of virulency of this alarming, this diſtreſſing, and, alas! too frequently fatal diſorder; I ſhall only add my moſt ſincere wiſhes that the *experience* of *others* may confirm the *hope*, which is founded on *my own*, *that the remedy* I now *recommend is poſſeſſed of ſpecific powers in the cure of this, if not of other malignant diſorders, like it, ariſing from contagion*; at the ſame time declaring, that ſuch have been the effects of this ſimple medicine, that were I

to be ſeized with the *plague itſelf* this moment, the *volatile alkali* would be the only remedy I would have recourſe to, and I ſhould fly to it with *confidence!*

A few Remarks, by Anticipation, on the Concluſion which will probably be drawn of the Nature of the Diſorder from the Quality of the Remedy.

As chemiſtry is the faſhionable ſtudy of the day, if I be favoured with half a ſcore readers, ſeven of them muſt be chemiſts; for at this time French principles are as neceſſary to the philoſopher as French faſhions are to a fine lady: neither of them can ſafely appear in public without them; conſequently, as being out of the faſhion is generally conſidered as like being out of the world, every man who wiſhes to be thought *knowing*, muſt be a chemiſt; and a chemiſt too of the antiphlogiſtic order.

One common remark and general concluſion will naturally occur to the gentlemen of this

order; which, as nearly as I can predict, will be in the following words:—" Umph!—If this be a fact, that the carbonate of ammonia counteracts, or deſtroys the contagion of ſcarlatina maligna; then, it muſt inconteſtably follow, that this *contagion* is an *acid*, and that the diſorder is in reality a ſur-oxygenation of the ſyſtem, which the carbonate of ammonia deſtroys, or removes, by combining with that acid to neutralization, and thus depriving it of its influence on the ſyſtem; by which means the ſyſtem is relieved from the diſorder occaſioned by the exceſs of oxygen, and health is reſtored."

If this be a fact, that the carbonate of ammonia is a ſucceſsful remedy in the ſcarlatina maligna, can any man be diſſatisfied with ſo natural a concluſion: a concluſion ſo pat and ſo truly chemical, that it ſeems to carry the fulleſt conviction along with it?—And yet there are objections, nay powerful objections to ſuch concluſion, and to any explanation which the modern principles of chemiſtry, conſiſtently with themſelves, can give.

It

It does not abſolutely follow that this contagion is an acid, becauſe ammonia deſtroys the effects it produces, and becomes neutralized by its paſſage through the body; neither is ſuch a mode of arguing either conſiſtent with reaſon or facts. For example, alcohol will combine with oxygen gas ſo as to form water. Many good old ladies are ſubject to *colicky pains* which are always *relieved* by a few glaſſes of *Nantz*, whoſe efficacy reſides in its *alcohol:* and this Nantz is converted into *water*. But are we then to conclude, that whenever one of theſe good old creatures has a touch of the colic, it proceeds from her having *fifteen* or *twenty gallons* of *oxygen gas* in her belly? Surely not. Though gas of ſome kind or other is no uncommon attendant in ſuch caſes, as is frequently evident to more ſenſes than one; yet, by whatever paſſage it makes its eſcape, it has never ſtruck me as being of the *empyreal kind*.

If the ſymptoms in ſcarlatina maligna are the effects of hyper-oxygenation, it is ſurpriſing that they have not been conſtant attendants in

many hoſpitals, and in many circles of private practice of late; ſeeing it has been ſo very much the *faſhion* to give the acids to an exceſs not known in former times; but, though hundreds and thouſands of patients have been ſur-oxygenated to the higheſt degree, yet, I never heard they were attacked with the ſymptoms of ſcarlatina maligna in conſequence. Neither does ſur-oxygenation render a perſon more *liable* to the diſeaſe, as I have had more than one indubitable proof of, during the time of the ravages of the diſorder now under conſideration. Nay, ſo far from that, in three caſes particularly, where the patients, otherwiſe in perfect health, were taking acids liberally for a cutaneous diſorder, *they eſcaped* the contagion, though their companions, who accompanied each of them in an occaſional attendance on the ſick, were *every one* of them *ſeized with the diſeaſe* on the third or fourth day after.

It is very evident then, that ſuper-oxygenation neither conſtitutes the diſorder, nor prediſpoſes the body to become ſubject to its influence.

How

How then can the contagion be an acid? and if it be, what is the ſtate of that acid?

Recourſe cannot be had here to the ridiculous argument of naſcent oxygen; becauſe, if naſcent oxygen means any thing, it muſt be oxygen in the act of ſeparation from one body, transferring itſelf to another; which of courſe muſt be immediately in *contact*. But breathing the air of a room where a patient in this diſorder is confined, without coming within three feet of the patient, has, in many caſes, inconteſtably, communicated the diſeaſe; which, on the third or fourth day after ſuch expoſure, hath ſo conſtantly appeared, as to render the time and mode of infection no longer problematical. Here the oxygen, if oxygen muſt be the cauſe of the diſorder, had not only been *ſeparated* from the infected body *ſome time*, but had *floated* in the atmoſphere to the *diſtance* of *three feet* at leaſt: it therefore could not be *naſcent*, ſtrictly ſpeaking, but was as *free* and *independent* as though it had been ſeparated *three years*, and had been *wafted* round the *globe*.

Perhaps

Perhaps it will be ſaid that this acid of contagion is not ſimply oxygen, but a *combination* of *oxygen* with a *peculiar baſe*, forming a peculiar acid, poſſeſſing the peculiar properties, to which we give the name of *contagion*; and that this acid of contagion has another peculiarity, which is, that, when received into the blood and acting as a contagion, it cauſes a *great quantity* of a ſimilar acid of contagion to be *generated*; as is peculiarly evident in ſmall-pox, from inoculation; when the *hundredth part of a grain* of the matter of infection will frequently produce as much infectious matter, *abſolutely ſimilar in all reſpects*, as can be contained in the *large puſtules covering the whole ſurface of the body!*

Admitting then that the matter of contagion is an acid, formed of oxygen and ſomething elſe; ſtill, that oxygen is not ſaturated with its baſe; for oxygen ſaturated is no longer acid: it ſtill then acts as oxygen, and is in reality oxygen, only partly or imperfectly combined with ſome peculiar baſe.

If

If then a ſmall portion of this acid of contagion be introduced into the circulation of a perſon in health, it cauſes an inconceivably greater proportion of a ſimilar acid of contagion to be generated; that is, it cauſes the *oxygen in the healthy blood* to combine partially with ſome *other principle of the blood*; and by forming this *combination* to ſeparate from the other principles of that blood they before were united with: for, as the perſon is in health, and the blood in a healthy ſtate, when the contagion is received; and as nothing but what is natural and conducive to health is taken in afterwards, till the contagious acid is formed, it is evident, that the *acid of contagion* thus *generated*, is actually *formed* from *certain principles ſeparated* from the *pure healthy blood itſelf*, of which they were a conſtituent part.

The queſtion now is, how does the oxygen of contagion *ſeparate* the oxygen of the blood from all its other combinations, except that portion of ſomething or other which renders it contagious alſo? By what law of chemical agency are we

to

to explain in what manner *one grain* of oxygen, combined with a certain baſe, acts upon the other principles of the maſs of blood in general, ſo as to make them *depoſit* a *thouſand grains* of *ſimilar contagion*, formed of oxygen with a certain baſe? In ſhort, why healthy blood, by the acceſſion of a *ſmall portion* of oxygen in a peculiar ſtate, ſhould be immediately diſpoſed to *depoſit* an *immenſely larger proportion* of oxygen in a ſimilar ſtate?

Theſe are queſtions, which the play of ſimple affinities between baſe and baſe, appears to me by no means calculated to anſwer; and conſequently the theory which is founded upon ſuch principles is not capable of giving that ſatisfaction which an inquiſitive mind muſt deſire, and ought diligently to ſeek after.

If oxygen, a name which originated in error, be a ſimple principle, it ought to be conſiſtent with itſelf. If then the matter of contagion be an acid, or a modification of oxygen, its properties ought to bear a reſemblance to the gene-

ral properties of all other acids or compounds where oxygen is not in a ſtate of ſaturation with its baſes: but that is not the caſe; for the acid of contagion cauſes a generation of a ſimilar acid in an aſtoniſhingly great proportion to the original exciting acid. This then is a ſtriking peculiarity which no other acid is poſſeſſed of. Indeed, were other acids endowed with the ſame generative powers when received into the living body, *ſome practitioners* of the preſent day might make *fortunes*, by merely preſerving the *droppings* from their patients.

If the matter of contagion be an acid, which produces the various ſymptoms of ſcarlatina maligna when it abounds in the ſyſtem, other acids, which are mere compounds of the ſame oxygenous principle in exceſs, when they are thrown into the ſyſtem in great quantities, ought to produce violent actions ſimilar to them: but they do not; and therefore, to ſay that contagion is an acid, and that its effects are produced by ſur-oxygenation of the ſyſtem, is ſaying nothing.

In ſhort, if oxygen be a ſimple principle, which, in its ſimpleſt, moſt diſengaged, and moſt condenſed ſtate, is *innocuous* when properly diluted, how does that inoffenſive principle become *virulent*, when its powers are *partly exerted in affinity* with a baſe of any kind? If the *full exertion* of its powers be *harmleſs*, how can a *partial exertion* of thoſe identical powers be *deſtructive?*

The ſame mode of reaſoning applies to the other principle, as a baſe of contagion. It is evidently derived from the blood; but every principle in the healthy blood, in its ſimpleſt and pureſt ſtate, is innocent, when taken into the body; how then can one ſimple *ſalutary* principle, by combining with oxygen, which is *equally* ſo, acquire *deleterious* properties, or form a compound which is poſſeſſed of *deſtructive powers*, when that combination is merely the effect of the ſimple principles *attracting each other?* By that attraction they cannot gain any power, or any new property; for when two bodies attract each other, *a part*, at leaſt, of their powers

are

are *mutually exerted upon each other*; and, conſequently, the only effect that combination can produce upon the powers of bodies or molecules, is, that as thoſe powers are in a great meaſure expended by acting upon each other, each body will have leſs power to act with upon other bodies.

Perhaps it will be alleged that the *bland* or *malignant* qualities of oxygen, or of the compoſition into which it enters, depends upon the *quantity* of *caloric* combined or connected with it:—indeed!

Oxygen, then, with a full doſe of caloric, conſtitutes oxygen gas, according to the modern doctrines. But, as that is the pureſt and moſt ſalutiferous of all gaſes, it *cannot* be ſuppoſed that the oxygen in the ſtate of contagion has any *ſurplus* of caloric; that would be deviating on the ſide of ſalubrity, and its effects ought to be highly invigorating, and ſimilar to thoſe produced by inhaling oxygen gas: therefore, as the effects of contagion are widely different, of

courſe, if caloric has any thing to do in the buſineſs, the *oxygen*, in *contagion*, muſt have *leſs than its common proportion of it*.

But, when oxygen leaves the infected body to paſs into the atmoſphere, why does it not take a ſufficient quantity of caloric along with it, as there can be no *ſcarcity* of it when it ſeparates from the *inflamed* ſurface of a body *ſcorching* with *fever*?

Probably by way of ſaying ſomething, or, in ſhort, any thing rather than give up a favourite idea, howſoever abſurd, it may be urged that the oxygen, forming the acid of contagion, is *deprived of its caloric* by its intimate combination with the baſe of that contagion; or that the acid of contagion is formed by the combination of oxygen with the peculiar baſe, *both* of which are *deprived* of their *caloric*.—Then the queſtion is, how is it poſſible for *one ſingle grain* of the acid of contagion to deprive *a thouſand grains of oxygen*, or of the *baſe of contagion*, or of *both*, of their *full proportion of caloric* which they *poſſeſs* in the *healthy*

healthy blood, and thus to *precipitate* them from the common maſs, in the *form* and with all the *virulent properties* of *contagion?*

Here, however, I ſhall drop the purſuit; having brought forwards a ſufficient number of objections to the modern modes of explaining the nature of diſeaſes, and the operations of medicines, to apologize for my *rejecting* them, and for my *boldly daring* to *think for myſelf*; for, while ſuch objections remain in full force, he muſt be fond of a theory indeed who can be pleaſed or ſatisfied with it; and they *muſt remain* in *full force* till they are *explained away* in a *rational* and *ſatisfactory manner*.

The greater number of thoſe who pretend to philoſophical knowledge are men who are not capable of thinking deeply (ſome from incapacity, and others from habitual indolence, and the degrading cuſtom of ſwallowing the *literary evacuations* of men of really great or pretended abilities, as *well-digeſted opinions*, without conſidering for a moment whether they are either *ſound* or *wholeſome)*; and for that reaſon they

are perfectly ſatisfied with any arguments which appear plauſible, howſoever ſuperficial. With philoſophers of this ſtamp, when opinion is once formed, every attempt to alter their mode of thinking, or root out that opinion, howſoever erroneous, is abſolutely in vain—they have made up their minds: they are ſatisfied that they are right: the greateſt philoſophers of the age have advanced and ſtill maintain that opinion; then, who can produce greater authority for a contrary opinion? If you attempt to reaſon cloſely upon the ſubject, they wiſely turn a deaf ear:—for, if they liſten they cannot underſtand; or *objections* are *not attended to*, becauſe they feel they have it *not in their power to anſwer them.*

I don't like principles that are too eaſy, too comprehenſive, too good-natured, and too ready to do or undo, juſt as occaſion ſuits. Every man who reaſons, ought to have principles upon which his reaſoning is founded; but thoſe principles ought to be ſimple in themſelves, ſimple in their properties, conſtant in their effects, and at all times, conſiſtent in their operations. But when

when principles are aſſumed which are hot or cold, fat or lean, wet or dry, conſiſtent or inconſiſtent, whichever is the moſt convenient at the time; ſuch ideal principles, or rather ſuch *terms without ideas,* are not the permanent, unchangeable principles of a philoſopher, but the *playthings of a child*; a *whiſtle* at one end, a *rattle* at the other!

Philoſophy is the faſhionable folly of the day, but what philoſophy? Is it the love of wiſdom, of knowledge, of facts converted into ſcience by reaſon? No. The philoſophy of the preſent day conſiſts in one continual waſte of uſeful articles, in trying ten thouſand trifling experiments, and repeating them ten thouſand times over, and ten thouſand times laying the jarring reſults before each other, and the public.

Let it not be thought, however, that I am an enemy to experiments, or ignorant of the great advantages which have been derived from them; on the contrary, no one is a greater advocate for them, when conducted with judgment, than myſelf; but, ſtill, *experiments* can but furniſh *materials*,

rials, which *reaſon* muſt convert into *order* and *ſcience*. *Experiments* are *ſteps* by which the philoſopher *aſcends* for the purpoſe of gaining a *more extenſive and diverſified view* of what ſurrounds him; but thoſe ſteps, however high they be piled, are *not wiſdom*: and the man who ſpends himſelf in heaping ſtep upon ſtep, is the mere *ſlave of ſcience*, who erects a *tower* from whoſe *ſummit* the *true philoſopher* contemplates at his leiſure the wonders of nature, thus *widely opened* before him.

I think I hear ſome one exclaim, What! are the principles of the French philoſophy ſo univerſally admired, and ſo generally adopted, are they not ſimple, conſiſtent, and intelligible? and is the beautiful theory ſo full of explanation, which is founded upon thoſe principles, a detail of experiments without reaſoning? Softly, and you ſhall hear, my good antiphlogiſtian! I have, ſo far, never advanced an opinion different from the common every-body's-way-of-thinking, without giving my reaſons for it; and as reaſons to ſupport ſuch an opinion are ready at every turn,

if

if you will give me leave, I will lay before you the firſt which come to hand.

As oxygen is a leading feature in almoſt every experiment which occurs, nothing more readily catches the eye than it. Let me then for a moment attend to what every body ſays, and every body believes, and every body is ready to maintain (except when they meet with arguments which may prove troubleſome) concerning the principle called oxygen.

Oxygen is a ſimple principle, according to the French ſchool, which poſſeſſes very ſingular properties. Sometimes it forms by combination with *bland* and *mild* principles, the moſt *acrid* compounds: as, for example, when by combination with *azote*, it forms *nitrous acid*.

Sometimes it combines with other mild ſubſtances, *without any acrimony*; as when with *hydrogen* it forms *water*: and ſometimes with the *moſt acrid*, *penetrating*, and *corroſive* of all things, it forms a *mild compound* of the moſt grateful and

ſalutary qualities; as when with caloric it forms *pure air*, or *oxygen gas!*

All *gaſes*, ſomehow or other, are *chiefly* compoſed of *fire*; and they take their names from the *ſmall*, the *very ſmall* proportion of the *principle* which in this ſomehow-or-other-way is combined with it; thus two or three grains of oxygen with a pint of caloric, form a pint of oxygen gas; and a grain or two of azote, with the ſame meaſure of caloric, conſtitute a pint of azotic gas.

Oxygen and azote thus by caloric converted into gaſes, have *no power of combination*, as is evident by mixing them together; when they ſtand all weathers, and every poſſible degree of compreſſion, without combining.

Nitrous acid is formed of oxygen and azote. If to this acid there be put a few filings of iron or copper, ſimple bodies, as the French teach us, the metal will attract part of the oxygen, when the azote, by means of caloric, expands into the ſtate of gas, taking a portion of oxygen along with it.

If

If this azotic gas, *charged with a portion of oxygen*, be mixed with oxygen gas, it will rapidly ſeize its oxygen; and the azote, by combination with that oxygen, and with that already attached to it, will form nitrous acid.

Or if this azotic gas, charged with a little oxygen, be added to a mixture of azotic and oxygenous gaſes, it will take the oxygen gas from the azotic, and with it will form nitrous acid; that is, they ſay, the azote not being ſaturated with oxygen in the ſtate of nitrous gas, will not only ſeize the oxygen of oxygen gas to ſaturation, but it will take *more* than it can *ſaturate*, and thus form a compound where oxygen evidently abounds; and this combination is not in any reſpect interrupted by each of thoſe principles being combined with caloric, and in the ſtate of gas.

Yet, a little while ago we were told, that azote and oxygen in the *ſtate of gaſes*, would *not combine*; and now we find, that in the *ſtate of gaſes* they *will rapidly combine*.

What reaſon is aſſignable for this palpable contradiction? The difference is, that *azote* in the ſtate of *nitrous gas*, is *already combined* with a *ſmall portion* of *oxygen*, and with *caloric* ſufficient to give it the ſtate of gas; but in ſimple azotic gas, the *azote* is *only combined* with *caloric*; if it can be called combination, where one thouſandth part of the caloric cannot be in contact with the azote.

Then *azote with caloric*, has *no ſenſible action* on the *oxygen of oxygen gas*; but if that azote with its caloric, be by any means *combined* with a *portion of oxygen*, it then becomes ſo *keen*, ſo *voracious*, ſo *inſatiable in its appetite to oxygen*, that it will *devour more* than it can *digeſt*; will ſeize upon *more* than it can *ſaturate*, in *deſpite* of *caloric*, and thus will conſtitute nitrous acid! Azotic gas, *already partly combined with oxygen*, then *will* attract oxygen in the ſtate of gas *rapidly*; while azotic gas itſelf has *no ſenſible attraction whatever* to oxygen in the ſtate of gas!

Surely

Surely it is a little extraordinary, a little irregular, a little inconſiſtent in azote, that, when combined with caloric in the ſtate of gas, it will not enter into combination with oxygen gas; but if by any means it be *crammed with a little oxygen*, when combined with caloric, its appetite then becomes ſo inſatiably voracious, that it will *gorge itſelf* with oxygen to *ſuper-ſaturation*. Yet, *ſuch* are the *doctrines*, *ſuch* the *principles*, *ſuch* the *beautiful theories*, ſo *full of explanation*, ſo *univerſally admired*, and ſo *generally adopted!* and ſuch the philoſophy of the day!!

It certainly is a matter of little conſequence to me, what are the prevailing opinions of the multitude; yet, I feel myſelf ſo ſincerely attached to truth, and ſo devoted to her ſervice, that I cannot avoid occaſionally pointing out what *appear to me*, evidently to be *errors*; that they may be carefully *inveſtigated*, and, if really errors—expunged: this, and this only, has been the motive which has induced me to write what I now offer on the ſubject; and I freely confeſs that I have undertaken it rather as a duty, than as having

ing any expectations that what I have written will be in *any degree attended to*, for reaſons which I have already more than once inſinuated.

Be that as it may; being from long and full experience convinced that the generally adopted modes of practice are neither founded on ſound principles, nor of ſuch decided efficacy in the greater number of diſorders as to inſpire the experienced practitioner with confidence; and being fully perſuaded that great improvements might and *ought* to be made, I for ſome time paſt have devoted all my time and all my attention to the ſubject, and my labours have not been wholly in vain.

Although it would certainly be greatly to my pecuniary advantage to keep as ſecrets the improvements which my experience perſuades me I have made in the treatment of ſome of the moſt formidable diſorders, yet no private views ſhall interfere with my firſt intention, which was, to direct all my endeavours to one great end—*the public good!*

This,

This, then, is the firſt communication, and I with confidence hope that it will be found of importance.

In a ſhort time it is my intention to lay before the public new methods of treating *inflammation of the bowels and ſtrangulated hernia.*

The unfortunate event which has ſo lately deprived the world of one of its nobleſt and greateſt ornaments, makes me regret that the mode of treatment which has ſo conſtantly ſucceeded in ſome of the moſt alarming caſes of the above diſorders which have occurred in my private practice, was not then communicated, as the practice is not only new, but experience has taught me, that beyond every other that I have heard of, it is alſo efficacious.

As this is the firſt of an intended ſeries of communications, I have thought it not improper to give this ſlight view of the reaſons which have compelled me to abandon the commonly-received theories, and now ſhall diſmiſs the ſubject;

ject; as, without alluſion to the theories of others, or to opinions of my own, I propoſe that my future communications ſhall be entirely *practical*, that being the readieſt way of accompliſhing my wiſh—*to be uſeful.*

THE END.

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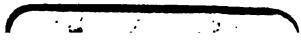
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